

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Social media interventions for diet and exercise behaviours: A systematic review and meta-analysis of randomized controlled trials
<b>AUTHORS</b>	Williams, Gillian; Hamm, Michele; Shulhan, Jocelyn; Vandermeer, Ben; Hartling, Lisa

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Paul Wicks PatientsLikeMe, United Kingdom
<b>REVIEW RETURNED</b>	25-Sep-2013

<b>GENERAL COMMENTS</b>	<p>The authors conducted a systematic review of the use of social media to promote diet and exercise in a general population. Where possible, meta-analysis was performed. The diverse mix of programs on offer suffered attrition, self-reported outcomes, and were broadly unconvincing. While this is not the authors' fault it makes it harder for them to make a compelling case here to warrant a reader's interest.</p> <p>Major comments</p> <ul style="list-style-type: none"> <li>• "Social media" is a broad church here, ranging from discussion boards to widgets and educational apps. I believe most readers will interpret "Social Media" to mean websites built from the ground up to be social, such as Facebook or Twitter. However many of the interventions here report use of a discussion board (which has been available in one form or another since the 1980's) or combination programs including face-to-face meetings and access to professionals as well. Therefore it feels like "Online interventions" might be a more fitting title than "social media" interventions, - the same applies throughout the manuscript.</li> <li>• The authors have done a systematic job of surveying the literature but I'm afraid they suffer from garbage in garbage out. Most of these studies are so small, so heterogeneous, and so poorly controlled that their outcomes are barely worth a meta-analysis, particularly when the outcomes are as mixed as weight loss, bone density, physical activity, and sweet beverage intake. A really mixed bag of what looks more like a series of pilots than properly powered studies.</li> <li>• Table 1 – Outcomes column should denote whether each outcome was reported objectively or as self-report.</li> <li>• Table 1 – Are the N's here the number enrolled or the number with complete data? If the former, the latter should be reported too for each arm to convey attrition.</li> <li>• Discussion – the authors should allude briefly to how this class of interventions compares in effect size magnitude to other types of interventions such as bariatric surgery, in person counseling, or weight-loss pharmaceuticals.</li> <li>• Discussion – the authors should make more specific</li> </ul>
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	<p>recommendations for the design of future studies in this area</p> <ul style="list-style-type: none"> <li>• Discussion – There is a vast iceberg of social media use by the general population that goes completely un-studied by academics. Whether tweeting about their latest run on RunKeeper, sharing the stresses of keeping to a diet on Facebook, or checking into the gym on FourSquare, what the academic literature can see seems to me to be so vanishingly small a window on the world as to be meaningless. Where is the data from Striiv? From Runkeeper? From FitBit? From WeightWatchers.com? This review feels more an indictment of academic abilities to construct attractive interventions than a failure of online interventions per se.</li> </ul> <p>I am not familiar enough with meta-analysis statistics to critique this portion of the paper.</p>
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<b>REVIEWER</b>	Ted Eytan MD Kaiser Permanente, United States
<b>REVIEW RETURNED</b>	29-Sep-2013

<b>GENERAL COMMENTS</b>	<p>Thank you for the opportunity to review this piece. It is timely and appropriate for our health system to understand the effectiveness of social media, which to implement in practice requires considerable resources, even if the platforms are provided at no cost.</p> <p>I commented on the references above because you might also reference the work of Bonnie Spring, PhD, at Northwestern, who authored a series of 3 papers regarding the use of mobile technology for behavior change, I've put those links together for you here:</p> <p><a href="http://delicious.com/tegyptan/spring">http://delicious.com/tegyptan/spring</a></p> <p>They reiterate what your reference 35 also appears to show, which is that technology by itself is not tied to success in these endeavors - a clinically guided program provided in tandem may be required.</p> <p>I believe this piece will be useful to health systems who are considering using this technology, to bring clinical expertise and behavior change science together for effective interventions.</p>
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<b>REVIEWER</b>	Annie Lau University of New South Wales, Australia
<b>REVIEW RETURNED</b>	03-Oct-2013

<b>GENERAL COMMENTS</b>	<p>This paper describes a systematic review on the use of social media interventions to promote the uptake and maintenance of healthy diet and exercise amongst healthy individuals. It is a well-planned and carefully executed study. Authors should address the following comments to clarify details in the Methods and Results sections, and further develop the Discussion to guide future research.</p> <p>[Methods]</p> <ol style="list-style-type: none"> <li>1. Authors need to provide the date of which the literature search was conducted.</li> <li>2. Authors also need to include the interrater scores (e.g. kappa) for the assessment of articles</li> </ol>
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	<p>[Results] 3. It would be a good idea to present a graph of how many RCTs there are in each type of social media</p> <p>[Discussion] While the authors have correctly discussed and concluded that social media interventions are constantly evolving, that the current forms do not appear to show significant differences in health outcomes/behaviours, and that high attrition rates and poor research reporting are problematic. Authors should provide further commentary to guide the future designs of social media interventions and their related research. For example:</p> <p>4. What type of social media components should we include in an intervention for healthy diets and exercise? Or in other words, how should we use social media in an intervention, and what types of social media are more likely to result in a significant impact? 5. Attrition rates are notorious in any e-health intervention. Can the authors comment on whether social media has a role in reducing attrition rates? Or how can we influence the design of the social media intervention or the study protocol to reduce attrition rate? 6. Are social media interventions more likely to succeed in particular types of population? 7. What details need to be included when reporting studies on social media intervention? (especially in RCTs) 8. Whilst the authors have correctly acknowledged that social media is constantly evolving, what advice do they give to those who work in the information technology field, such that the next generation of social media innovations can be designed in a way that would have more relevance to healthy diet and exercise?</p>
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<b>REVIEWER</b>	Nicole Ezendam Tilburg University, Tilburg, The Netherlands
<b>REVIEW RETURNED</b>	08-Oct-2013

<b>GENERAL COMMENTS</b>	<p>The paper “Social media interventions for healthy diet and exercise: A systematic review and meta-analysis of randomized controlled trials” is nice and clearly written, and reports on an important topic. The work is thoroughly and well conducted.</p> <p>In general, I’ve two questions/comments.</p> <p>1. Could the authors say something about the of the follow-up duration after the study? And also the duration of the intervention? This topic is touched in the discussion section, but how are the results for the studies in this review?</p> <p>2. Did the authors compare the results of the intervention with the discussion boards with the interventions using Facebook/twitter. These are rather different kinds of interventions and it would be of interest to know if only the discussion board interventions show little/no effects (and lack of adherence). Are facebook/twitter like intervention, using a medium that is regularly used by part of the population, more effective? More specific, I would like to ask the authors to reply to the following questions/comments. Page 5 “No beneficial effect of social media on lifestyle behaviour</p>
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	<p>change was found.” I do not understand this comment since you did find a positive effect on dietary fat intake. Can you please clarify this conclusion?</p> <p>Page 9 “these were calculated using a correlation of 0.5 if not provided in the study report.” Why did the author choose for this correlation. Was is supported by other studies or their own findings?</p> <p>Page 10 “We planned to assess for publication bias visually and graphically in the meta-analysis with the most contributing studies using Egger’s test.” Could the authors clarity what this test is in one or a few sentences?</p> <p>Page 15. “The use of these strategies in conjunction with social media interventions might result in increased success.” Reading the review, I don’t see how this statement is supported by the results of this review paper. Please clarify.</p>
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<b>REVIEWER</b>	Alan Batterham Research Professor, Teesside University, UK.
<b>REVIEW RETURNED</b>	10-Nov-2013

<b>GENERAL COMMENTS</b>	<p>This is a rigorously-conducted review of social media interventions. I have a number of issues for the authors to respond to/ clarify.</p> <ol style="list-style-type: none"> <li>1. I suggest changing the title to “... for diet and exercise behaviours...” or some such. I am uncomfortable with the term ‘healthy’ particularly as the review includes dietary fat consumption. With respect to total fat/ fat as a percentage of energy intake all robust lines of evidence indicate that it is unimportant clinically for chronic disease risk. Rather, specific dietary fatty acids are causally implicated in chronic disease (i.e. CVD) but total fat is largely irrelevant.</li> <li>2. My main concern surrounds the metric used for the standardised mean difference, and I would like to see the methods/ justification made more explicit. The authors have extracted the change from baseline for intervention and control arms and, apparently, have used the SD of the change scores as the denominator. They assumed a conservative reliability of <math>r = 0.5</math> in those studies where the SD of the change scores or the correlation between pre- and post- measures was not presented. It is unclear to me which between-subject standard deviation has been used to compute the SD of the change scores using the assumed reliability of 0.5. Have you used the baseline SD, the post- SD or some composite? The baseline SD is likely more appropriate as the post SD can be inflated by individual differences in response to intervention. Please clarify.</li> <li>3. Related to the above, I would like to see a robust justification for using the SD of the change scores as an appropriate metric to assess magnitude of effect herein. As discussed in detail by Cumming and Finch (Educational and Psychological Measurement, 61, 532–574, 2001), to assess the size of an effect by standardising the effect by the standard deviation of the change scores does not make substantive sense. Arguably, the natural frame of reference for thinking about changes due to an intervention is the estimate of the population standard deviation indicated by the between-subject variance in the sample at baseline. Please comment and provide a justification for your chosen metric vs. alternatives.</li> </ol>
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## VERSION 1 – AUTHOR RESPONSE

Reviewer #1

Reviewer comments:

“Social media” is a broad church here, ranging from discussion boards to widgets and educational apps. I believe most readers will interpret “Social Media” to mean websites built from the ground up to be social, such as Facebook or Twitter. However many of the interventions here report use of a discussion board (which has been available in one form or another since the 1980’s) or combination programs including face-to-face meetings and access to professionals as well. Therefore it feels like “Online interventions” might be a more fitting title than “social media” interventions, - the same applies throughout the manuscript.

Author’s response: Thank you for this comment. While most studies included in the review were in fact based on discussion groups, we were still considering these tools to fall under the definition of social media, as it is felt that these are a precursor to social media. We established our definition of social media (including discussion boards) prior to beginning this review. Our intention was to review all social media tools used, and the fact that discussion boards emerged as being so predominant in the literature was one of the results, rather than one of our objectives. Our intent was not to capture all “online interventions.” This encompasses much more than discussion boards; therefore, we feel that it would be misleading to use this broader term.

Reviewer comments:

The authors have done a systematic job of surveying the literature but I’m afraid they suffer from garbage in garbage out. Most of these studies are so small, so heterogeneous, and so poorly controlled that their outcomes are barely worth a meta-analysis, particularly when the outcomes are as mixed as weight loss, bone density, physical activity, and sweet beverage intake. A really mixed bag of what looks more like a series of pilots than properly powered studies.

Author’s response: We agree with this comment in many respects. However, we felt that it was important to examine the literature to identify both its strengths and limitations. One of the values of a comprehensive systematic review is indeed to identify some of the limitations so that these can be addressed in future research. We feel that we have identified issues that should be addressed both in the development of interventions involving social media, as well as their evaluation.

Reviewer comments:

Table 1 – Outcomes column should denote whether each outcome was reported objectively or as self-report.

Author’s response: We have added whether each outcome was reported objectively or as self-report to table 1.

Reviewer comments:

Table 1 – Are the N’s here the number enrolled or the number with complete data? If the former, the latter should be reported too for each arm to convey attrition

Author’s response: We have added the number enrolled, the number with complete data and the number of participants analyzed to table 1.

Reviewer comments:

Discussion – the authors should allude briefly to how this class of interventions compares in effect size magnitude to other types of interventions such as bariatric surgery, in person counseling, or weight-loss pharmaceuticals.

Author’s response: We have added into the discussion comments about how the interventions we examined compare to other similar interventions including commercial weight loss programs and transtheoretical models using stages of change models. While there are reviews of bariatric surgery, we feel that the intervention and the target group is vastly different and doesn’t provide an appropriate comparison to interventions that are based on behavior change.

Reviewer comments:

Discussion – the authors should make more specific recommendations for the design of future studies in this area

Author's response: We have added recommendations based on limitations of the studies included in this systematic review.

Reviewer comments:

Discussion – There is a vast iceberg of social media use by the general population that goes completely un-studied by academics. Whether tweeting about their latest run on RunKeeper, sharing the stresses of keeping to a diet on Facebook, or checking into the gym on FourSquare, what the academic literature can see seems to me to be so vanishingly small a window on the world as to be meaningless. Where is the data from Striiv? From Runkeeper? From FitBit? From WeightWatchers.com? This review feels more an indictment of academic abilities to construct attractive interventions than a failure of online interventions per se.

Author's response: We agree that social media is being used extensively for many and varied purposes; however, if these haven't been formally evaluated and reported on, we are unable to include the data in a systematic review. We have added to the discussion a reference to a systematic review that examined commercial weight loss programs, this included studies of Weight Watchers.

Reviewer #2

Reviewer comments:

I commented on the references above because you might also reference the work of Bonnie Spring, PhD, at Northwestern, who authored a series of 3 papers regarding the use of mobile technology for behavior change, I've put those links together for you here:

<http://delicious.com/te dey tan/spring>

They reiterate what your reference 35 also appears to show, which is that technology by itself is not tied to success in these endeavors - a clinically guided program provided in tandem may be required.

I believe this piece will be useful to health systems who are considering using this technology, to bring clinical expertise and behavior change science together for effective interventions.

Author's response: Thank you for your suggestions. We have incorporated two of the papers into our discussion. The third paper mentioned by the reviewer was a protocol was not included.

Reviewer #3:

Reviewer comments: [Methods]

1. Authors need to provide the date of which the literature search was conducted.

2. Authors also need to include the interrater scores (e.g. kappa) for the assessment of articles

Author's response: 1. In our methods section under "search strategy" we specify that the search was conducted from 2000 to April 2013. We have added the date that the librarian ran the search, i.e., May 3, 2013.

2. We don't have this information as this review was part of a much larger project examining social media across all healthcare topics. We have published numerous systematic reviews (over 30) and have never provided the interrater scores, as we feel it is more important that a consensus process is established to discuss and resolve differences. We have described the study selection process as recommended by the PRISMA reporting guidelines for systematic reviews ([www.prisma-statement.org](http://www.prisma-statement.org)).

Reviewer comments: [Results]

3. It would be a good idea to present a graph of how many RCTs there are in each type of social media

Author's response: Thank you for your suggestion. We have added a graph depicting this to the results section, see figure 2.

Reviewer comments: [Discussion]

While the authors have correctly discussed and concluded that social media interventions are constantly evolving, that the current forms do not appear to show significant differences in health outcomes/behaviours, and that high attrition rates and poor research reporting are problematic. Authors should provide further commentary to guide the future designs of social media interventions and their related research. For example:

4. What type of social media components should we include in an intervention for healthy diets and exercise? Or in other words, how should we use social media in an intervention, and what types of social media are more likely to result in a significant impact?

Author's response: 4. We do not feel that we can answer this question at this time. No social media component stood out as having a significant impact.

Reviewer comments:

5. Attrition rates are notorious in any e-health intervention. Can the authors comment on whether social media has a role in reducing attrition rates? Or how can we influence the design of the social media intervention or the study protocol to reduce attrition rate?

Author's response: 5. We found high attrition rates even with social media interventions. Moreover, many authors commented on challenges of keeping the participants engaged. We think an important message for those considering using social media as part of an intervention is to not assume that they will keep the target audience engaged; this may be a common assumption because social media is generally so popular and widespread. We have added a comment about this to the discussion.

Reviewer comments:

6. Are social media interventions more likely to succeed in particular types of population?

Author's response: 6. Again, we did not find any examples of where the interventions were particularly "successful"; therefore, we cannot answer this question directly. One recommendation we can make is that those developing an intervention should gather feedback from the target audience during its development in order to optimize its likelihood of uptake and success. We have added this to the discussion.

Reviewer's comments:

7. What details need to be included when reporting studies on social media intervention? (especially in RCTs)

Author's response: 7. We have added some details on this to the limitations' section in the discussion and provided a reference to a standard reporting guideline for randomized controlled trials (i.e., The Consort Statement).

Reviewer comments:

8. Whilst the authors have correctly acknowledged that social media is constantly evolving, what advice do they give to those who work in the information technology field, such that the next generation of social media innovations can be designed in a way that would have more relevance to healthy diet and exercise?

Author's response: 8. As we mentioned above, we believe that it is of utmost importance to involve the target audience when developing the intervention in order to optimize uptake and use. We have

added this to the discussion.

Reviewer #4

Reviewer comments:

1. Could the authors say something about the of the follow-up duration after the study? And also the duration of the intervention? This topic is touched in the discussion section, but how are the results for the studies in this review?

Author's response: For all studies, except one, the length of follow up was the same as the length of the intervention. In the one study, follow up data was not used. We have added this detail to Table 1.

Reviewer comments:

2. Did the authors compare the results of the intervention with the discussion boards with the interventions using Facebook/twitter. These are rather different kinds of interventions and it would be of interest to know if only the discussion board interventions show little/no effects (and lack of adherence). Are facebook/twitter like intervention, using a medium that is regularly used by part of the population, more effective?

More specific, I would like to ask the authors to reply to the following questions/comments.

Author's response: 2. Unfortunately, we there is not enough data to tease this out as most did not show a strong effect.

Reviewer comments:

Page 5 "No beneficial effect of social media on lifestyle behaviour change was found." I do not understand this comment since you did find a positive effect on dietary fat intake. Can you please clarify this conclusion?

Author's response: Page 5: Thank you for this comment. The statement has been modified.

Reviewer comments:

Page 9 "these were calculated using a correlation of 0.5 if not provided in the study report." Why did the author choose for this correlation. Was is supported by other studies or their own findings?

Author's response: Page 9: We apologize for this confusion as this statement was written in error. We have changed this to clarify what was done: "we divided by the standard deviation of the final value SD (or the baseline SD when the final SD was not given)."

Reviewer comments:

Page 10 "We planned to assess for publication bias visually and graphically in the meta-analysis with the most contributing studies using Egger's test." Could the authors clarify what this test is in one or a few sentences?

Author's response: Page 10: We have added two sentences to clarify what the Egger test is

Reviewer comments:

Page 15. "The use of these strategies in conjunction with social media interventions might result in increased success." Reading the review, I don't see how this statement is supported by the results of this review paper. Please clarify.

Author's response: Page 15: The statement has been revised.



Reviewer #5

Reviewer comments:

1. I suggest changing the title to “... for diet and exercise behaviours...” or some such. I am uncomfortable with the term ‘healthy’ particularly as the review includes dietary fat consumption. With respect to total fat/ fat as a percentage of energy intake all robust lines of evidence indicate that it is unimportant clinically for chronic disease risk. Rather, specific dietary fatty acids are causally implicated in chronic disease (i.e. CVD) but total fat is largely irrelevant.

Author’s response: 1. Thank you for your comments. We agree and this change has been made.

Reviewer comments:

2. My main concern surrounds the metric used for the standardised mean difference, and I would like to see the methods/ justification made more explicit. The authors have extracted the change from baseline for intervention and control arms and, apparently, have used the SD of the change scores as the denominator. They assumed a conservative reliability of  $r = 0.5$  in those studies where the SD of the change scores or the correlation between pre- and post- measures was not presented. It is unclear to me which between-subject standard deviation has been used to compute the SD of the change scores using the assumed reliability of 0.5. Have you used the baseline SD, the post- SD or some composite? The baseline SD is likely more appropriate as the post SD can be inflated by individual differences in response to intervention. Please clarify.

Author’s response: Thank you for pointing out this lack of clarity. As above, we have clarified what was done with the following sentence: “we divided by the standard deviation of the final value SD (or the baseline SD when the final SD was not given).”

Reviewer comments:

3. Related to the above, I would like to see a robust justification for using the SD of the change scores as an appropriate metric to assess magnitude of effect herein. As discussed in detail by Cumming and Finch (Educational and Psychological Measurement, 61, 532–574, 2001), to assess the size of an effect by standardising the effect by the standard deviation of the change scores does not make substantive sense. Arguably, the natural frame of reference for thinking about changes due to an intervention is the estimate of the population standard deviation indicated by the between-subject variance in the sample at baseline. Please comment and provide a justification for your chosen metric vs. alternatives.

Author’s response: We have modified the analysis section as described above in order to clarify what was done.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Ted Eytan Kaiser Permanente, United States
<b>REVIEW RETURNED</b>	26-Dec-2013

<b>GENERAL COMMENTS</b>	I agree with one of the other reviewers' comments that this piece will highlight the lack of high quality research in this area, which is why its contribution is important. Thanks for the opportunity to review.
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<b>REVIEWER</b>	Annie Lau Centre for Health Informatics, Australian Institute of Health Innovation, University of New South Wales
<b>REVIEW RETURNED</b>	07-Jan-2014

<b>GENERAL COMMENTS</b>	The authors have adequately addressed reviewers' questions and suggestions.
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<b>REVIEWER</b>	Dr. N.P.M. Ezendam Tilburg University, the Netherlands
<b>REVIEW RETURNED</b>	06-Jan-2014

- The reviewer completed the checklist but made no further comments.

<b>REVIEWER</b>	Alan Batterham Teesside University, UK
<b>REVIEW RETURNED</b>	07-Jan-2014

<b>GENERAL COMMENTS</b>	In my view the authors have addressed the reviewers' concerns adequately.
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